

Peer-review report of

Marinthe, G., Lalot, F., Kasper, A., Manunta, E., & Brown, G. (2026). Collective action hinders the increase in post-election trust among election losers: Longitudinal evidence from the 2024 UK election. *advances.in/psychology*, 1, e411811.
<https://doi.org/10.56296/aip00048>

Round 1

Editor Decision

Dear Authors,

Thank you for submitting your manuscript, "Collective Action Hinders Trust Amelioration Among Election Losers: Longitudinal Evidence from the 2024 UK Election," to our special issue on The Psychology of Pushback. We have now reviewed your work independently and then received insightful commentary from two longstanding experts in the field. This decision is based on their constructive feedback and our own editorial assessment.

The manuscript has clear and compelling strengths. Both reviewers recognized its value, with Reviewer 1 finding your research idea "valuable and relevant." Reviewer 2 highlighted that "the article's most important conceptual contribution is its integration of two separate research traditions," calling the perspective "both novel and theoretically significant." We as editors agree and were particularly impressed by the cohesive argumentation in your introduction and the manuscript's overall high quality of writing.

Despite this strong foundation, both reviewers identified several limitations that require careful attention. The reviewers are very thorough and detailed in their comments, and they see similar weaknesses (as well as strengths) in the current version of your work. Their feedback converges on key areas that we also viewed, in our independent reviews, as critical to address. We therefore invite you to submit a revised manuscript that thoroughly engages with these points. Many of these points involve methodological and analytic aspects of the work and thus are critical to the meaning of the findings currently reported. The reviewers have offered extensive and thoughtful feedback; rather than reiterating every point, we will highlight the overarching issues we believe are most crucial for revision.

Both reviewers raised valid questions about your collective action measure, noting that it combines several distinct types of actions. Please clarify your implicit measurement model (e.g., reflective vs. formative). Even if the model is formative, we recommend supplementing it with psychometric analyses to explore the measure's underlying structure. This analysis may reveal distinct types of collective action, and if so, it would be important to explore their potentially different effects. We also urge you to address the scale's distributional challenges. Please test your mixed-model assumptions (the dharma package is useful for this) and report bootstrapped analyses (e.g., via the bootMer function) if you find substantial

deviations. Reviewer 2 offers various ways to address the non-normal distribution. As a robustness check, you might also consider dichotomizing the scale (0 = no collective action; 1 = at least one action).

We concur with the reviewers regarding the deviation from your pre-registered plan for the trust measure. As editors, we find your chosen measure compelling and understand the decision to deviate. However, to strengthen the manuscript, please test for measurement invariance over time, as Reviewer 1 suggests. It could also be insightful to explore findings using the in-group party trust items. For full transparency, please report the analyses with the original, pre-registered items in the supplementary materials and briefly summarize the results in the main text.

Your analyses require further clarification. Please specify the components of your mixed models as requested by Reviewer 1, including which intercepts and slopes were set as random effects and the justification for these decisions. You must also report the software and packages used, including version numbers and citations. To bolster the robustness of your findings, please consider Reviewer 2's suggestion to include covariates known to be associated with political trust. Furthermore, as Reviewer 1 notes, a more nuanced discussion of statistical power is needed, and care should be taken to resolve potential inconsistencies in reporting.

We encourage you to elaborate on the precise theoretical mechanism underlying your main hypothesis. Why, exactly, should engagement in collective action lead to decreased trust among those whose preferred party or candidate loses? This mechanism should be detailed more thoroughly in the introduction to fortify your hypotheses and revisited in the discussion to propose future work that could directly test these pathways. In doing so, please acknowledge the limitation raised by Reviewer 2 that these mechanisms remain speculative until empirically tested.

While the introduction and discussion are already strong, they would benefit from a dedicated paragraph explicitly connecting your work to the special issue's theme. Please articulate how your findings on trust and collective action speak to broader issues of democratic backsliding and what lessons can be drawn for understanding the psychology of pushback.

In addition to these larger points, please address the remaining specific comments from the reviewers and the following items.

METHODS

Please specify the content of the attention checks.

Please specify the scale format and endpoints for all measures.

RESULTS

Figure 4 currently uses a continuous line for a binary predictor (time), which is not ideal. Please replace this with point estimates and 95% confidence intervals. We encourage you to keep the jittered individual data points, which are helpful.

DISCUSSION

Please revise statements that imply the absence of an effect without a direct test of the null hypothesis. For example, instead of stating that an increase in trust “was not observed,” rephrase to note that the effect was not “statistically significant” among a particular group. Please carefully check the manuscript for other similar instances as there were a few more.

Please remove the “Constraints on Generality” subheading, as a heading level requires at least two subheadings and this part is already captured in the overarching heading.

We found the following statement confusing: “Although we observed an overall increase in political trust moderated by electoral outcome, we were unable to establish a clear baseline for this increase.” Is Time 1 not this baseline? While experiments are indeed better for establishing causality, the baseline measurement does not seem to be the critical limitation here.

STYLISTIC ISSUES

Please present all p -values with three digits (even if the last number is a 0), unless $p < .001$.

Please carefully revise all citations and references for accuracy. For instance, the author “and Neyazi” is cited but their full name is “Taberez Ahmed Neyazi”; a similar issue occurs with “and Allen.” Also note that both the word (“and”) and the symbol (“&”) are used (by mistake) in that Neyazi citation.

In summary, we believe your manuscript has considerable promise but has not yet reached its full potential. We invite you to submit a revised version that addresses the points outlined here and by the reviewers. Recognizing that these revisions will require additional space, we have increased the word limit for your submission by 2,000 words. Given the fundamental nature of the changes requested, the final determination will depend on how effectively you were able to address the concerns raised about the current manuscript and the impact of your revisions on the findings that emerge.

Please submit your revision by October 20, 2025. If you require an extension, please do not hesitate to contact us.

We look forward to receiving your revised manuscript.

Best regards,

Jonas R. Kunst & John F. Dovidio

Reviewer 1

Thank you for inviting me to review the manuscript, “Collective Action Hinders Trust Amelioration Among Election Losers: Longitudinal Evidence from the 2024 UK Election,” for possible publication in *Advances.pub*.

The study addresses an important and timely question: whether pre-election collective action moderates changes in political trust following the 2024 UK election. Drawing on a two-wave panel survey ($N = 543$), the authors report a general post-election increase in trust, a familiar winner–loser gap, and—at the local level only—a three-way interaction in which pre-election collective action appears to negate the trust increase otherwise observed among local “losers”.

While the idea is valuable and relevant, I find the manuscript in its present form to have serious problems that compromise the robustness of the claims.

First, I have substantial concerns about the construct validity and scoring of the collective action index. The checklist conflates heterogeneous behaviours, mixes national and local targets, and restricts recall to “in preparation for the election,” which respondents may interpret unevenly. Items that are essentially status/lifetime decisions (e.g., party membership) are folded into a “past month” window, producing near-zero endorsement alongside transient campaign activities. The theoretical range is also misreported (the text describes 11 scorable actions plus an “other” option, yet states 0–12), and the distribution shows severe floor/zero inflation ($M \approx 0.52$; $SD \approx 1.00$), calling into question the use of a z-scored linear predictor. I did not find psychometric evidence or discussion of dimensionality. Ultimately, the recoding of “other” actions lacks coder reliability and a transparent rubric.

I would invite the authors either to justify their methodological choices or to adjust their analysis to make it more rigorous (reporting endorsement rates and reliability ($KR-20/\alpha$) at Wave 1; testing structure via EFA/CFA or IRT and, if heterogeneity is evident, forming conceptually coherent subscales (e.g., low- vs high-cost; national vs local) rather than a single sum). Addressing these points will materially strengthen the validity and interpretability of the key moderator.

Second, I have major concerns about the measurement of political trust. The manuscript departs from its preregistered two-item measure and constructs a post hoc six-item composite using separate EFAs at each wave, without subsequently establishing longitudinal measurement invariance. This raises three problems: (1) the deviation from preregistration is insufficiently justified and blurs confirmatory vs exploratory evidence; (2) the retained items blend heterogeneous targets—politicians in general, MPs, and local council—yet the paper later tests moderation by local vs national electoral outcomes; this target mixing risks criterion contamination and level-of-analysis mismatch; (3) the factor-analytic workflow (wave-specific EFAs with oblimin rotation) identifies additional factors (party/party leader; a third/failed item), but these are then ignored while forming a single composite, with no CFA, no report of factor correlations/communalities/variance explained, and reliance on α alone (no ω/H). The result is a composite whose stability across time and alignment with the theorised construct are not demonstrated.

To improve transparency, the authors might consider: (a) presenting the

preregistered two-item results as the primary analysis, clearly labelling the six-item scale as exploratory; (b) including an appendix with item texts, coding (including reversals), loadings, communalities, factor correlations, and retention criteria for both waves; (c) evaluating longitudinal measurement invariance via CFA (configural, metric, scalar) and reporting ω alongside α ; (d) addressing target heterogeneity by analysing separate national and local trust composites or, alternatively, excluding local-target items from the national composite and showing robustness across specifications; and (e) indicating whether the main findings replicate when the party/party-leader items are modelled explicitly (e.g., two-factor, bifactor/ESEM) or when those items are omitted.

Third, I found multiple conflicting statistics that undermine confidence in all results. For instance, local outcome sample size and missingness are not documented: the authors classify local winners ($n = 181$) and losers ($n = 279$), totalling 460—well below $N = 543$ —without explanation (unmatched constituencies? missing postcodes?). Yet later tests for local effects use degrees of freedom (df) around 456 and 540, creating further confusion. I invite the authors to provide a CONSORT-style flow for local matching and a missing-data analysis. I recommend re-estimating the results on clearly defined analysis populations.

Fourth, I appreciate the real-world constraints behind sample planning, and it is helpful that feasibility considerations are acknowledged. That said, the current power rationale feels a little aspirational. It relies on “feasibility/budget” plus a general citation, and the stated detectable d for the three-way interaction is not anchored to concrete assumptions (e.g., pre–post correlation of the outcome, intraclass/cluster structure, random-effects specification, df method). From a reader’s perspective, it would be reassuring to see how power changes once those design features are considered—especially because three-way interactions in mixed models are notoriously sensitive to these parameters.

A light, simulation-based power (and sensitivity) analysis tailored to the final model would go a long way. I encourage the authors to simulate using the observed pre–post correlation, variance components/ICCs, the actual group sizes and balance, clustering (respondent/constituency), and the df method used (Satterthwaite/Kenward–Roger). Including brief code/output and a compact table or plot showing power across plausible ranges (e.g., interaction β , ICC, $\pm 10\%$ attrition) would make the justification feel both transparent and grounded. If a full simulation is impractical, a post hoc sensitivity analysis—reporting the smallest interaction effect detectable at 80% power given the realised design—would still meaningfully strengthen the manuscript.

Fifth, in my view the model specification is under-reported: “mixed model” is stated, but the random-effects structure (random intercepts for person; random slopes for Time), estimation method, covariate structure, and df method (Satterthwaite/Kenward–Roger) are not described. Centering/coding is partially reported (Time, Outcome = $-1/+1$; CA z-scored), but not for all predictors. Without this, dfs and SEs cannot be evaluated; clustered data may be mishandled.

I kindly invite the authors to provide a more accurate specification of the models; perhaps opt for an ANCOVA-style model ($\text{PostTrust} \sim \text{PreTrust} + \text{Outcome} \times \text{CA} +$

controls + (1|ID)) and report robustness to different reasonable random-effects structures.

Sixth, there is causal language overreach in the current version of the manuscript. The authors claim “real-world, causal evidence,” yet their paper’s design is observational pre–post without randomisation, exogenous assignment, or a control group beyond time. This should be reframed as associational evidence; any causal claims require stronger identification. (The authors’ own limitations section partly acknowledges this, but the title, abstract, and conclusions still overstate.)

Finally, a note on interpretation and framing. As currently written, the narrative seems to promise a symmetric, “amplifying” pattern across levels. Yet the results indicate that the national three-way interaction is null, while the local three-way interaction is significant (subject to the reporting corrections noted earlier). It may help to temper the general claims and state explicitly—especially in the Abstract, Introduction, and Discussion—that the evidence supports a level-specific (local-only) pattern rather than the full hypothesis at both levels.

A very minor final suggestion: consider revising the language around “backfire”. The Conclusion repeatedly implies that collective action “backfires”. Given the current measurement and reporting uncertainties, the absence of multiplicity control, and potential confounds, this reads as firmer than the evidence can sustain. A conditional, local-level exploratory framing would be more proportionate at this stage.

Given these issues, I cannot recommend the paper for publication in its current form. A thorough re-analysis with corrected reporting, explicit preregistered vs exploratory distinctions, measurement validation, and clarified modelling would be required before the substantive contributions can be adequately assessed.

I appreciate the opportunity to review this manuscript and hope these comments will be helpful to the authors and the editorial process.

Reviewer 2

This manuscript makes a timely and original contribution by examining whether pre-election collective action moderates the well-established winner–loser gap in political trust following the 2024 UK General Election.

The article’s most important conceptual contribution is its integration of two separate research traditions: (1) the well-established literature on the “winner–loser gap” in post-election trust, and (2) the psychology of collective action, which is usually studied as an antecedent of political outcomes rather than as a determinant of post-election attitudes. By asking whether pre-election mobilization moderates how winners and losers adjust their trust after an election, the authors open a new explanatory pathway: collective action as a potential amplifier of the emotional and attitudinal consequences of defeat. This perspective is both novel and theoretically significant.

An important conceptual strength is the paper’s distinction between national and local outcomes. Many studies in political psychology focus exclusively on national-level winners and losers, but the authors highlight the importance of proximity and

attribution—that people may evaluate whether their efforts “paid off” in their own constituency, which is more psychologically immediate. This framing allows the paper to speak to multi-level processes of trust formation and connects with broader debates about the role of local representation in democratic legitimacy.

The authors argue that electoral losers who engaged in collective action may fail to recover trust in the political system, particularly at the local constituency level, where outcomes are most psychologically salient. Using a two-wave panel design, they show that while political trust increases after the election overall—especially among winners—this rise does not occur among engaged local losers. The paper thus expands the literature on electoral trust by integrating insights from research on collective action and by emphasizing the importance of local outcomes in democratic legitimacy.

A methodological strength is the implementation of a natural experiment design (surveying individuals just before and just after the 2024 UK General Election). It provides strong ecological validity and overcomes limitations of using mostly cross-sectional designs. The transparent preregistration and open materials further reinforce the study’s credibility, reducing concerns of data mining.

The analytic strategy, using mixed-effects models to test interactions across time, outcome, and collective action, is appropriate and carefully reported. The results are presented clearly, both using tables and figures to plot the interactions. The pattern of results revealing that trust rises after the election, especially for winners, but not for engaged losers at the local level is easy to follow and is supported by figures and model outputs. The authors are careful not to overstate the null finding at the national level, noting instead a more general negative correlation between collective action and trust among non-Labour voters. However, the effect sizes are modest, and more emphasis on standardized differences (e.g., change in SD units) would help readers grasp their substantive significance. Figures illustrating the key interactions improve interpretability.

The discussion section is insightful and well-structured, successfully linking the results to broader theories of electoral trust and the psychological consequences of losing. The emphasis on local outcomes as the key site where collective action matters is compelling and aligns with existing theory on proximity and attribution. However, the section relies heavily on speculative mechanisms (procedural fairness, efficacy) that were not measured, leaving the explanatory account incomplete. Alternative explanations—such as partisan differences in expectations, attrition bias, or differential salience of local contests—are mentioned only briefly and deserve fuller exploration.

Several aspects should be addressed to strengthen and improve the article. First, I recommend rewording the hypotheses section to make each prediction explicit and clearly labeled (e.g., H1, H2, H3). This would help readers follow the logic of the study and understand in advance what the authors expect to find. In my view, the prediction of a three-way interaction only became clear once I reached the results section. In addition, I suggest referring back to the corresponding hypothesis when presenting the results, and explicitly indicating whether each prediction was

supported, partially supported, or not supported. This would substantially improve clarity and readability.

Second, report the scales used in the several measures of trust reported in Table 1 (the note in the Table 1 might be a good place to do it). Are these comparable to create a single composite (aggregate measure)?

Third, a measurement adaptation post-preregistration was implemented. Originally the preregistered trust measure was a two-item scale, but the authors report results with a six-item composite derived from exploratory analysis. Although this is justified as capturing a broader construct, it opens researcher degrees of freedom and weakens preregistered confirmatory claims. Robustness checks with the preregistered measure would help address this. In addition, why authors didn't perform a confirmatory factor analysis instead based on conceptual grounds?

Fourth, regarding the collective action measure, the distribution is strongly skewed, with most participants reporting zero or one action. This limited variance undermines the ability to detect moderating effects and conflates heterogeneous forms of action into a single index. Weighting by effort or distinguishing online vs. offline action might produce a more valid measure. I suggest addressing this issue in the discussion section for future research to consider.

Fifth, with only two waves, the study cannot establish whether trust changes are temporary or enduring. The design also does not allow one to disentangle whether the observed effects are driven by winners increasing their trust, losers decreasing theirs, or both processes simultaneously. I recommend expanding the discussion to underscore the importance of incorporating at least three waves in future research, in order to capture trajectories of change over time and to address the issue of potential order effects more directly.

Last but not least, it would have been valuable to include controls for factors that are well-established predictors of political trust (e.g., ideology, populist attitudes, national identity, demographics). As far as I can tell, these variables were not incorporated into the models, which raises concerns about potential omitted variable bias in the reported interactions.

In conclusion, this is a well-designed and innovative manuscript that advances understanding of the interplay between collective action and electoral trust. Its central claim—that collective action can hinder trust recovery among local losers—has both theoretical and policy relevance. Addressing the issues raised above will considerably strengthen the paper and enhance its contribution to political psychology and democratic research. I therefore recommend revise and resubmit.

Round 2

Editor Decision

Dear Authors,

Thank you for submitting your revised manuscript, "Collective Action Hinders Trust Amelioration Among Election Losers: Longitudinal Evidence from the 2024 UK

Election," to our special issue on *The Psychology of Pushback*. We have reviewed your work independently and sought further commentary from the two experts who evaluated the previous version.

We concur with the reviewers that you have done an excellent job revising your manuscript. You have rigorously addressed our concerns, and as a result, the manuscript is now very close to acceptance. We only request that you implement the following set of minor revisions:

Writing and Structure

- Please implement the linguistic suggestions made by Reviewer 1, as we agree these will increase the accessibility of the paper.
- On page 24, please remove the term "real-world," as this is implied. Instead, you can describe it as "longitudinal survey data."
- Please add a fifth keyword after the abstract (we suggest "longitudinal") and organize the keywords alphabetically.

Statistical Reporting and Methods

- Thank you for providing the library names and versions. Please also cite the lme4 package, as we believe lmerTest is based upon it.
- You state that participants were included as a random factor; however, in the regression syntax provided, they are included as a random intercept. Please revise the text to reflect this accurately.
- Thank you for providing scale endpoints for the trust measure in the table note. Please move this information into the main text where you describe the measure.
- Please unblind the ethics committee details and provide the approval number. Additionally, please make the OSF repository openly available and update the link.

Figures and Tables

- Figure 1: This figure consumes excessive space relative to the information it provides and is not strictly necessary. Please remove it and instead summarize its content in 1–2 sentences (i.e., election results and changes from the previous one). Make sure to update all table and figure numbering.
- Table 1: Please move Table 1 to the supplement and briefly summarize the presented distribution in the text instead.

- Figures 3 and 4: The y-axis should start with the minimum possible value on the scale (likely "1") rather than being truncated. Please also ensure the table notes are updated to reflect the new location of the former Table 1.
- Figure 5: Please present means and 95% CIs (similar to Figure 3) instead of boxplots. Additionally, the legend label "Group judged" is not intuitive; using *Election outcome* would be better to maintain consistency in presentation.
- File Format: Please ensure all figures are included in SVG format to guarantee high resolution. These must be directly exported as SVGs (e.g., from R or PowerPoint) and inserted into the document; converting from another image format to SVG is not appropriate.

References and Special Issue Cohesion

- We are striving to create cohesion within our special issue by highlighting connections between articles. Please consider how the first two published papers (<https://advances.in/psychology/10.56296/aip00042/> and <https://advances.in/psychology/10.56296/aip00040/>) could inform your arguments and discussions.
 - For instance, on page 3, when you discuss the erosion of democratic engagement and the rise of opposing forces, Borghi et al. (2025) may be relevant as it was also conducted in the UK and describes this development.
 - Similarly, Lalot & Abrams (2025) analyze the same event (2024 UK General Election) and find a similar "winner-loser" or "ingroup" bias in how voters forgive leaders. Crucially, they also find a general increase in forgiveness post-election, which parallels your finding of a general increase in political trust.
- Please use italics in the reference list in line with APA 7 standards. For instance, journal titles (and volume numbers) and book titles (but not chapter titles) need to be italicized.
- There is a DOI missing for Citrin & Green (1986). Please add: <https://doi.org/10.1017/s0007123400004518>

Submission Requirements

- Please provide the Supplementary Materials as a Word file to facilitate processing during production.

Given the minor, presentational nature of the requested changes, please submit your revision by **December 23, 2025**. If you require an extension, please do not hesitate to contact us.

We look forward to receiving your revised manuscript.

Best regards,

Jonas R. Kunst & John F. Dovidio

Reviewer 1

Dear Authors,

thank you for the opportunity to read your revised manuscript. My overall impression is very positive: the paper is theoretically clear, methodologically careful, and makes a timely contribution to the literature on political trust and collective action. At this stage, I see it as essentially ready for publication, with only very minor language polishing needed.

The main point I would invite you to reconsider is the recurrent use of the expression “trust amelioration,” including in the title, which is not very idiomatic in English. A formulation such as “increase in political trust” or “post-election increase in political trust” would read more naturally and would also better capture the idea of an attenuated *increase* in trust among highly engaged local election losers.

Beyond this, the remaining issues are very small copy-editing details (for example, “corner stone” could simply become “cornerstone”, and “discarded for the local level analyses” might sound smoother as “excluded from the local-level analyses”), as well as a quick check for tiny typos in the references. None of these points require substantive changes, and I am happy to recommend the manuscript for publication once these minimal stylistic edits have been addressed.

Reviewer 2

Review of the revised version of the manuscript: Collective Action Hinders Trust Amelioration Among Election Losers: Longitudinal Evidence from the 2024 UK Election

I would first like to explicitly congratulate the author(s) for the excellent review process carried out in revising the article, particularly in how they addressed the various conceptual and methodological aspects suggested by the reviewers. The rigor, clarity, conciseness, and well-founded nature of their responses to each of the recommended points are truly remarkable. I must highlight this as a central strength, as it demonstrates the seriousness with which the authors engaged with the recommendations in order to add value to this important publication. I sincerely commend them—such a thorough (and admittedly extensive) revision process is rare, and it clearly reflects strong academic motivation and a genuine commitment to

improving the manuscript. Below, I outline the most relevant aspects that emerged from my previous evaluation of this manuscript (and it does not address the points raised by the other reviewer, whose feedback I also found highly informative).

1. Clarity and explicitness of the hypotheses. Even though the authors only partially addressed my observation regarding the hypotheses and the possibility of numbering them, their argument that the hypotheses were formulated in that way in the preregistration leads me to think that they are correct. What is noteworthy is that, following the reviewer's recommendation, they clarified the hypotheses section of the manuscript and added explicit statements in the results section indicating whether the hypothesis was supported or partially supported.

2. Description and comparability of the trust measure: in the revised manuscript, the authors provide much more detail on the political trust measurement. They specify that nine items were used, describe the exploratory factor analyses that yielded a six-item "political system trust" factor and a two-item ingroup party trust factor, and report reliability indices for the six-item composite at both waves. They also indicate in figure notes that political trust is measured on a seven-point Likert scale. Although the snippet of Table 1 is not fully visible in the text extract, Table 2 and the figure notes make clear that the trust items share a common response format (7-point scales), and the factor-analytic treatment justifies the construction of a single "political system trust" composite.

3. Post-preregistration measurement adaptation and robustness checks: The revised manuscript now describes, in detail, the steps taken when the preregistered 2-item measure turned out to be weakly correlated. The authors explain that they had preregistered the possibility of using alternative trust items, report separate EFAs at both time points, and derive a coherent six-item factor for political system trust. They also conduct a longitudinal measurement invariance analysis of this six-item construct, finding configural and metric invariance and partial scalar invariance—essentially a longitudinal CFA with invariance testing. Crucially, they now explicitly state that they re-ran the analyses using the preregistered 2-item measure and that those results are reported in the Supplementary Material. They also cross-reference these robustness analyses in the analytic strategy and in the local-level results, indicating, for example, that the three-way interaction is only marginally significant when the preregistered 2-item measure is used. Summarizing, the authors now provide a transparent account of why they deviated from the preregistered 2-item measure, conduct appropriate factor and invariance analyses consistent with the reviewer's CFA suggestion, and provide preregistered-measure robustness checks.

4. Skewness and heterogeneity of the collective action measure: I argued in my assessment that the collective action index was strongly skewed, with most participants engaging in zero or one action. I noted that this limited variance and

conflated heterogeneous actions into a single index and recommended that this limitation be discussed and that alternative operationalizations be considered (e.g., weighting by effort, distinguishing offline vs. online action). Robustness analyses were conducted. In the results, they report further analyses using (a) a dichotomous indicator of any vs. no collective action, and (b) a Rasch score that incorporates item difficulty (implicitly weighting more costly/rare actions). They show how the key interactions behave under these alternative operationalizations and note, for instance, that the three-way interaction at the local level becomes non-significant when using the dichotomous indicator, though the pattern of simple effects remains similar. I think this robustness analyses added value to the manuscript.

5. Two-wave design and the temporal dynamics of political trust: The limitations section now explicitly acknowledges that the two-wave design precludes strong causal inference and recommends either experiments or longitudinal designs with more than two waves. The authors also explicitly note that designs with more waves would allow researchers to distinguish intra-individual from inter-individual change, for example via random-intercept cross-lagged panel models, and to determine whether post-election fluctuations in trust are short-lived or long-lasting. While they do not dissect in detail whether the observed net changes are more strongly driven by winners' increases vs. losers' decreases, they do emphasize in the general discussion that trust increased for both groups, but more strongly for winners, which already clarifies this issue. The temporal limitations and the need for richer longitudinal designs are now clearly articulated and linked to appropriate modelling strategies.

6. Omitted-variable bias and lack of key covariates: In the analytic strategy the authors now report that they estimated additional models including several covariates—gender, age, and national British identification—and that the inclusion of these covariates did not materially change the results. These models are reported in the Supplementary Material; the main text presents the more parsimonious specification without covariates.

7. Precision and scope of the discussion: In the revised version, the general discussion is conceptually rich and clearly articulated. The authors more explicitly mark their speculations—for example, they now note that any mechanism involving procedural justice remains “speculative” given the available measures. The limitations section is expanded and more systematic, including discussion of the generality of the trust measure, the low level of collective action, and the temporal limitations. Indeed they explicitly note the overall low level of engagement in collective action in their sample, explain that they used an additive behavioral scale among “average citizens” rather than activists, and suggest that future work use scales with more response options (e.g., “never”, “once or twice”, “more than twice”) and/or focus on more activist samples to increase variation at higher levels of

engagement. Summarizing, the authors both acknowledge the skewed nature of the measure and conduct additional analyses using more sophisticated operationalizations that speak directly to the reviewer's worry. The discussion has been strengthened and made more precise, especially in how limitations and speculation are handled.

8. Conclusion and recommendation to the editor: the authors have responded seriously and substantively to the critical points raised in the initial review. The most important methodological concerns—regarding the change in the trust measure, the skewness of the collective action index, and the limitations of the two-wave design—have been carefully and transparently addressed through additional analyses (factor analyses, invariance testing, robustness checks with alternative operationalizations and the preregistered measure) and through an expanded limitations section that acknowledges design constraints and outlines appropriate future directions. Conceptually, the hypotheses are articulated more clearly than before, and the discussion is richer and more nuanced, with clearer connections to the literatures on political trust and collective action. On balance, I judge that the authors have addressed the central substantive and methodological critiques in a satisfactory way. I would therefore recommend acceptance